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ABSTRACT

If drama theorists are to improve the profession at large, they need to put their own house in order first. Too often drama theorists rely on intuition, tolerating the unexplained and the unsystematized. They could learn from theorists in other fields, such as physics and the "hard" sciences, by organizing the facts of their field of knowledge and by realizing that the formulation of theories is a cyclical process of analysis, synthesis, and verification. Organizing the facts known about drama, understanding these facts thoroughly, and using them to improve drama theory is what drama theorists should be doing. In this way they make their own work and work in the general field of drama both easier and more productive. But preparation of theorists must first begin with learning the facts upon which valid, effective theories are built. In short, drama theorists need to focus on the analysis, synthesis, and verification of the facts of drama. (RL)

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"A FACT WITHOUT A THEORY: the role and responsibilities of the quantitative researcher in dramatic theorizing"

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I see two major problems in the theatre profession, both of which will be solved (if they are solved) by theorists:

1) We (the profession at large) are willing, nay eager, to trust our intuitions fully. Not only do most of us deny the need for objective verification of our perceptions and belief; many among us also deny the possibility of verification. And I must go still further: many of us fear the possibility that we might answer some of our questions conclusively. After all, one of the major "comforts" of our trade is the fact that most of us (once we have our Ph.D.'s) are never put to the test, neither in our art nor in our opinions. So long as all opinions are of equal value and we decline to go beyond opinion, to search for fact, we are safe. But we pay a dreadful price for this comfort. The quantitative researcher has a huge sales job to do if he is to arouse concern for his work.

2) Our colleagues also have a great tolerance for the unexplained and the unsystematized. They are often content with an understanding of theatre loaded with black boxes and they too often entertain contradictory opinions without noticing it. To put it simply, they don't really understand what Theory is all about. They doubt its importance and its reliability. They have settled for the unexamined life which Plato assured us was not worth living. The theorists, generally, have a big job to do, and it won't be an easy one. But before we can expect to reform the profession at large, to persuade them to attend to us, to care about our work, to trust us, we must put our own house in order.

The feeling of urgency which moves me in these remarks is the result of my impression that the field of Dramatic Theory, which seems to me to be due for a period of great advancement, has taken a step which could undermine our efforts.

It is useful, when considering the work of theorizing to conceive it as involving three kinds of work which I call Analysis, Synthesis, and Verification. This set of distinctions, like so many others, is useful only in our minds; it is dangerous when it gets out into the world of action. Any substantial theoretical problem necessarily requires coordinated functioning in all three modes. Now here is the problem: Pandora has opened the box and these three useful tools have escaped and taken up residence in individual bodies. We now, in fact, have Analysts, Synthesizers, and Verifiers, each trying to carry on independently as if he could do the job alone. It doesn't work. The effect is that Theory, in the largest sense, has fulfilled little of its promise and individual workers in the three areas have been little rewarded for their efforts, either in cash or in the knowledge that their work has served their profession.

The problem is so pervasive that, very often, scholars specializing in one of these tasks don't realize that they are, like it or not, theorists (or at least a part of a theorist), that the quality and usefulness of their work depends on their understanding of the other two aspects of the task.

To set things up so that you will be able to understand my argument, I will briefly explain "Theory" as I understand it.

Theory is the "science" of any discipline (be it Physics or Drama). Science is, after all, just the systematic effort to explain what is going on. Theorists aim to describe and explain, or, more accurately, to construct a systematic rationalization of our experience. Theory tries to explain:

1) what is going on,
2) what has gone on,
3) what might go on.

I divide the complex job of theorizing into three fundamental kinds of work: analysis, synthesis, and verification. These tasks (which are only fully successful when carefully integrated) are often the private preserves of different

individuals:

- Analysis is the basic work of the Critic.
- Synthesis is the major work of the person we call (unfortunately) the Theorist.
- Verification is the most common work of the person we call (again unfortunately) the Empiricist.

Let's consider the task of each of these:

- The Analyst specializes in identifying parts, stages, and phases, and in spotting patterns among them.
- The Synthesizer refines our conceptual tools and sets out the rules for their use. He is the builder of explanations and guardian of our comprehensiveness and consisten . His main mode of reasoning is Deductive.
- The Verifier refines the systems of controlled observation and the systematizing of data and sets out rules for their use. His basic tool is what is usually called Induction but is more accurately known as Hypothetico-deductive Reasoning.

All are theorists. A great range of skills is required to carry these three tasks out well and to know the place of each in the over-all effort to understand.

Our major limitation as Theorists is Epistemological naiveté. We're all engaged in the game of describing and explaining. Epistemology is the study of the rules of that game. It tells us what we can do and how. The sub-divisions of this field may sound more familiar: Semantics, Logic, the Rules of Evidence, Cognitive Control Systems, Cybernetics, Problem-solving, etcetera. We must master them all, no matter which of the theory jobs appeals to us most. Otherwise we will continue to be limited by Synthesizers whose work is never put to the test of experimental verification and may, therefore, be dismissed as "just opinion", and Experimenters whose work is wasted because it is never integrated into the

larger picture or because meticulous control of data is undermined by inadequate conceptual work.

The naivete and lack of technique that I complain of is common to workers in all three categories, but I'll concentrate here on the problem as it relates to those I'll call (for lack of an adequate label) "quantitative researchers" or "experimenters", that is, on those whose main efforts are in controlled observation for the purpose of verification.

It's not surprising that experimenters are often underprepared for the analytic and synthetic aspects of their work. The nature of quantitative work tends to distract us from the larger theoretical issues. So much of our time goes into the gathering and manipulation of data that the basic purpose behind our efforts (i.e. the integration of hard fact into our reasoning on the basic issues of understanding) is often lost.

In our training of quantitative researchers, we tend to focus too exclusively on experimental technique and statistical analysis. This isn't hard to understand. It is evident to all of us that, without these skills, the job can't be done, and our students usually come to us completely ignorant of these techniques. What should be equally obvious to us, but usually isn't, is that analytic and synthetic skills are equally essential and that we don't come by these skills spontaneously. Experimentation tends to pay off only when it springs from the perceived needs of those who try to deal with the larger issues of theory and when the findings are integrated into the larger picture. The skills which would enable us to "finish" our work (that is, to bring our research to full fruition on the stage and in the classroom) are generally neglected and, so, much of our effort lies wasted.

Also, the temperament which leads one into a particular area of the work often makes one impatient with the others. Synthesizers are most at home when dealing with large scale structures. High-level abstractions are their normal mode of thought. They tend to be impatient with the systematic drudgery of

statistical analysts. The reverse is true for many experimenters: those abstractions seem too shifty, too uncertain; we want something we can touch, see, feel sure of.

But we can't settle for this typical response. Theoretical work does require both modes of work and the scholar who can work in only one is hamstrung.

The flaws in our experimental work are not usually flaws in those techniques we think of as peculiarly experimental (e.g. statistical analysis, controls, etc.) but, rather, in the control of concepts and the drawing of implications. We too often have trouble knowing:

a) what needs to be said,
b) what we have said,
c) what we have proven,
d) what its consequences are,

and these flaws are specifically analytic and synthetic.

Most of us chuckle at the foolishness of our ancestors when we recall that Physics (the hardest of the hard sciences) was once a branch of Philosophy. Was that so foolish? Physics was necessarily Philosophy before modern techniques of experimentation made it possible to add vigorous verification techniques to the fundamental philosophical techniques. Add, not substitute. Physics is still highly philosophical.

But we tend to forget the necessary philosophical aspect in our infatuation with the mechanics of experimentation. Philosophy, after all, is really nothing but our effort to explain what is going on and to develop the cognitive tools of investigation and explanation. There is no meaningful Science without Philosophy, or to put it in more immediate terms, without Theory. Newton and Einstein are synthesizers before they are investigators and verifiers.

Speaking of Einstein, let me share this with you from The Evolution of Physics, a book he wrote with Infeld:

"It is really our whole system of guesses which is to be either

> proved or disproved by experiment. No one of the assumptions can be isolated for separate testing. ...[Scientific] concepts are free creations of the human mind, and are not, however it may seem, uniquely determined by the external world. In our endeavor to understand reality we are somewhat like a man trying to understand the mechanism of a closed watch. He sees the face and the moving hands, even hears its ticking, but he has no way of opening the case. If he is ingenious he may form some picture of a mechanism which could be responsible for all the things he observes, but he may never be quite sure his picture is the only one which could explain his observations. He will never be able to compare his picture with the real mechanism and he cannot even imagine the possibility or the meaning of such a comparison. But he certainly believes that, as his knowledge increases, his picture of reality will become simpler and simpler and will explain a wider and wider range of his sensuous impressions." (pp. 30-1)

Let me repeat the first thought for emphasis: "It is really our whole system of guesses which is to be either proved or disproved by experiment." This is the thought of a fully rounded theorist and a man of great epistemological sophistication. It is also a staple notion in contemporary Philosophy of Science. But it is not part of the way most of the Experimenters in Dramatic Theory see it. Too often we are what scientists call Naive Empiricists. This poor creature makes the mistake of approaching his scientific work without first casting off the "common sense" view of the world which scientists disposed of long ago, the view that things <u>are</u> as they <u>seem</u>, which ignores the effect of world-view, the influence of experience on perception, the distortion of perception and judgement caused by our conceptual schemes, expectations, etcetera.

The Naive Empiricist believes in pure Induction and he aims for an explanation which is <u>fully</u> verified. He believes that if he looks long, hard, and carefully at the world and then systematizes his perceptions, he will eventually accumulate an explanation of his experience.

The world and theory just don't work this way. For a start, the bulk of any

comprehensive theory is, in principle, unverifiable. It is composed of convincing but untestable hypotheses about why things behave as the experimentalists verify they do.

Most progress in Theory is made in one of these two ways:

Approach 1:

- A Synthesizer formulates a Hypothetical construct, a tentative explanation of some aspect of experience.
- This construct is reduced to testable hypotheses.
- The hypotheses are confirmed or disconfirmed by a Verifier, experimentally.
- Implications of the findings are integrated into the Hypothetical Construct or the construct is abandoned.

Approach 2:

- A question occurs to a researcher.
- He translates it into an hypothesis or set of hypotheses.
- The hypotheses are tested experimentally.
- The implications of the findings are pursued and may suggest new hypotheses.
- Eventually the theory to which the findings are related is either modified, supported or cast off.

In other words, the process may begin at either end, with the Synthesizers great hypothetical leap or with the Verifiers nagging question, but in either case, the process is cyclical. All phases of theory-making are involved. Wherever the cycle begins, it is not likely to produce much of value unless the full sequence is completed, and this is the problem which too often hamstrings us. Because so few of us have a thorough understanding of, and basic skills for, each of these functions, most of our work ends up as just more academic litter and the profession goes its way, still shaped predominantly by uncontrolled impressionism.

Much work can be done at any point in the cycle, but until the full circle is complete, we haven't finished our work.

Historically, the most valuable experimentation has been aimed at confirming or disconfirming extant theory. For example, Einstein's two great hypothetical

leaps, his explanations of relativity and of the relation of energy to mass. These had their birth as full-blown explanations. Verifiers then set to work to reduce these explanations to testable hypotheses. The hypotheses were confirmed and, viola', we have space travel and nuclear power. But the Verifiers had to have a very sophisticated understanding of Synthetic skills in order to understand the implications of the explanation and to devise the crucial hypotheses. That these conclusions would have been inferred from the systematic collection of data without that great hypothetical leap is very unlikely.

To return to the matter of naivete' from another angle: beyond skill in the design of experiments and in inference-from-data (and before it, preferably) the researcher needs a fundamental understanding of the thecretical (philosophical) underpinnings of his work.

He may not reasonably "leave the theory to the theorists" because everything about his procedure is based on certain philosophical assumptions. For example, a quantifier subscribes (implicitly, at least) to the Materialist/Empiricist/Pragmatist philosophical orientations. He denies Rationalism and Idealism. But most experimentalists (in my limited experience) have not come to grips with these issues. They assume the Materialist/Empiricist/Pragmatist orientation as if it were an unquestionable fact of life. Most of us reject the Idealist/Rationalist orientation without ever having heard the arguments for either side. This isn't safe and it leads to such problems as inconsistency (e.g. I've known many experimentalists who simultaneously operate on self-contradictory bases, being rigorously Empiricist and Materialist in their experimental work and Idealist in many of their uninvestigated beliefs). For example, I know good quantifiers who also hold the belief that there is such a thing as Tragedy and our job is learning what it is. Is it clear that in these two moments they are operating on contradictory belief systems (Materialism and Idealism)? It won't wash. The only defense against such errors is conscious awareness of the theoretical underpinnings of our work and skill in consciously noting the epistemological status of all statements and ideas.

Both synthesizers and experimenters must avoid the mistake of believing in absolute and knowable laws of nature. All such laws are presuppositions, the assumptions which we infer (consciously or not) from whatever cosmic egg we happen to be operating in. Scientists used to say that the principle of inertia, for example, was a "fundamental law of nature, the most universally true assertion in dynamics." Now they reject that way of putting it and say, instead, inertia is the most generally applicable principle of interpretation guiding our observation and explanation of dynamics, (cf. my section on "Unity" in Understanding Play-scripts). We must not fail to notice such presuppositions or we are liable to miss better explanations of our experience than those which our presuppositions cause us to see as "evident."

I fear we are also too little aware of the whole issue of conceptual schemes. Concepts are the primary tools of all theorists and the schemes into which we organize them (our models, parodigms, world-views, etc.) are the most powerful and insidious of the forces influencing the quality of our work.

Concepts are the primary tools in all three aspects of theoretical work. The Experimenter cannot work "uncorrupted"by conceptual schemes. He cannot perceive and name his experiences without having already committed himself theoretically. When the researcher decides to compare the effects of Inclusive and Preclusive directing styles, he has already committed himself not only by presuming that controlled observation is the apt way to answer the question but also, for example, by assuming that this distinction is meaningful and relevant. These presumptions are "given" by a theoretical orientation, whether we're aware of the orientation or not. This is only a problem if we don't know about it, accept it, and allow for it. All theoretical workers are concept and world-view dependent in this way, so we had better become fully sensitized to their threat and their amazing potential.

And, one more example of the subtleties of our task which require epistemological sophistication. We tend to play free and easy with the whole notion of

fact. We are too little aware of the relativity of that notion. The current view of facts (current, that is, in the thought of those whose primary concern is to deal with such matters, the philosophers of science) is that a fact "is" a fact only with reference to a coordinate system, a frame of reference, a model, a theoretical construct. What we call a fact is basically determined by the theoretical structure within which investigation and experiment occur. And theoretical structures are purposive, i.e. a theory can be said to be an adequate explanation only relative to some purpose for which the explanation is formulated. This relativistic orientation is behind the explosion of progress in the hard sciences.

To put it differently, a "statement of fact" (as we call them) is a mini-theory which claims that it is generally useful to speak in this way about this aspect of our experience. No absolute claims are made; the fact is not only theory-bound and purpose-bound but also situation-bound. The criteria for accepting a statement are always to be found in the specific situation and purposes which generated the investigation. We understand now that "facts" are statements about reality and do not necessarily capture that reality. Utility, which we once saw merely as a criterion for action, is now seen to be a valuable part of our criteria for statements of fact.

This is just an example, of course, just one of the hundreds of crucial matters in theoretical work. My point is that we dare not be naive on such matters or our conclusions will be knocked down as fast as we set them up.

I complain of limitations common among quantitative researchers not to denigrate the work or the workers but because I respect it so highly and because we all must depend on it so fully. I want to see its potential fulfulled.

I'm convinced that theatre is just another human activity, that it has no special, ineffable characteristics which make it forever mysterious, and that it can be understood in the same ways we understand any other human activity. But it is disconcertingly complex and will be understood only to the degree that we

are subtle in our epistemological foundations, rigorous and ingenious in our investigations, comprehensive and pragmatic in our goals.

The job won't be done without the efforts of an army of skilled quantitative researchers who have great theoretical sophistication--far more than we have today. And only the generally acknowledged "success" of those already in the trade is likely to draw others to it.

What would constitute "success"? I think it will include at least these factors:

- indisputable evidence that
- something which concerns the profession is now
- reliably explained in such a way that
- our work is likely to be easier or more productive.

As you see, this required us to go far beyond the careful gathering and analysis of data. It requires it to choose the right questions, to pursue them comprehensively, to report them clearly and to persuade our readers to note the implications. It requires, in short, fully-trained and highly strategic theorists. And that is what we must make of ourselves or resign ourselves to remain in a pedantic backwater of our art.

My hope is that this job will be done by theorists who are fully rounded, fully prepared in analysis, synthesis, and verification. My pragmatic compromise (for the short term only) is the appeal for the balanced theoretical team, working together on every aspect of the job. The interaction among members of this team, must be constant. Each participates in all of the fundamental design and interpretation work. Each serves as a safeguard and stimulant to the others. Explanations change as controlled observation provides anomalous data. New hypotheses require the development of new techniques of observation. New questions suggest new methods of statistical analysis. New tools of observation suggest new questions which provoke new hypotheses. This interaction can produce great excitement and new insight and it certainly enhances our chances of producing meaningful and

trustworthy work.

We may be on the threshold of the first highly productive era in the history of dramatic theory. It will be so if all of us are ready to pay the high price of preparation.

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